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its severe storms multiplied by reporters to too great an extent. It seems necessary to give these figures exactly as they came out; and I shall be very much gratified if the citizens or professional men of any State, feeling that their State has not been given a fair show, should make for themselves a careful canvass of the State for tornado losses. I shall be pleased to send to any such State a list of the dates of all the tornadoes reported from that State, for the authorities or those interested to verify the losses sustained. I have attempted, by correspondence and in other ways, to get more complete returns, but there seems to be great apathy on this question. Possibly many have been very much discouraged at the palpable exaggerations that have been published. It seems to me that this matter is of the greatest importance, and now is the time to establish an accurate estimate of tornado losses which will be of permanent value for comparison with future years. Certainly more harm will come from vague reports of doubtful tornadoes and exaggerated statements of losses than can ever come from an exact knowledge of the truth. There is no doubt that there is a tornado evil. Let us learn its exact proportions, and then people will know just what to prepare for.

I do not think that in this table the States of New York and Pennsylvania can be regarded as full-fledged tornado States. Moreover, the comparative loss between fire and tornadoes is not the same as in the other States, owing to the relatively greater loss by fire in the cities in these two States. I have therefore taken out these two States in the final summing-up of the table. Kansas has been left in, however. We find that in the fifteen remaining States the relative loss was one-fourteenth; that is to say, the loss by tornadoes is seven per cent of that by fire. I think we may safely say that any fire insurance company would be entirely secure if they increased their premium by eight per cent or ten per cent, and assured the householder against loss from tornadoes as well as fire, provided such insurance could be placed through all the fifteen States, and for a term of four or five years. As will be shown later, there seems to be good evidence for believing that once in eleven years the risk from tornadoes is somewhat increased for two or three years. When we see that insurance companies, computing from imperfect tornado losses multiplied by 25, have charged the same for both fire and tornado insurance, it is not to be wondered at that so few have taken the latter. Every one has the evidence of his senses that tornado losses in his community in fifteen to twenty years, excepting a few of the more disastrous tornadoes, have not equalled the average fire loss in a single year. It is said that such matters generally adjust themselves through a competition and rivalry between different companies, but it is very unfortunate for a few who have to suffer while this adjustment is taking place. Will it not be far more satisfactory to insured and insurers if this subject be thoroughly ventilated, and a good idea of the comparative risk between fire and tornadoes be arrived at? It should not be forgotten that it is only by uniform action and support of both fire and tornado insurance by the people in the fifteen States, that any thing like an average result can be obtained; or, at least, this must be the case in any one State, for there are a few lines of action in the State which seem to be more favorable for the development of tornadoes than the whole area of the State. H. A. HAZEN.

M. PASTEUR AND HYDROPHOBIA.¹

IT is now five years since M. Pasteur introduced to the medical world his alleged cure for hydrophobia. If his much-vaunted discovery possesses all the merits which have been claimed for it, he has earned a fair title to the gratitude of mankind. If, on the other hand, it can be shown that all his theories depend for their acceptance upon a number of very serious fallacies, and that his alleged cures are no cures at all, inasmuch as in those cases the disease never existed, and yet further, that in many cases his treatment has actually induced hydrophobia where it was previously non-existent, M. Pasteur's claim to be regarded, on account of this treatment, as a benefactor of his race, must fall to the ground.

It is now thirteen years since I first wrote a series of articles for the *Medical Press and Circular* on this subject, and they were subsequently published in book form under the title "Rabies and Hydrophobia." On that occasion I pointed out a very serious fallacy underlying many cases of alleged cure of this disease,—the fallacy of regarding persons bitten by healthy dogs as in danger of hydrophobia. At that time I investigated carefully a number of cases of alleged cures by a clergyman residing near Burnley, who had a great reputation in Lancashire for the cure of hydrophobia. The result of my inquiry showed that the Rev. Dr. Verity, the clergyman in question, had had a large number (two thousand) of dog-bitten patients. A few of them had died from hydrophobia after his treatment; but the majority escaped, the reason being that they had been bitten by non-rabid dogs, or had been bitten through clothing, etc. I inquired into numerous other alleged cases of cure of hydrophobia, but always with the same result; and I was thus led to formulate this proposition,—'that if any one obtained a reputation for the prevention of hydrophobia, and if all the dog-bitten sought or took this remedy, the result would be statistically favorable.'

When M. Pasteur startled the world by his first statistics, I was disposed to believe that in the hands of a man of such great scientific fame a cure had been found. I could not, however, avoid noticing the same fallacy running through his statistics which characterized the returns made by the Rev. Dr. Verity. The dog-bitten were certainly rushing to M. Pasteur; but the results were robbed of much of their marvellous character if it could be shown, that, owing to panic and fashion, great numbers bitten by non-rabid dogs were seeking protection.

This objection was supported by another, very powerful consideration. I found, on carefully comparing the statistics given by M. Pasteur with those of the years which preceded the introduction of his system, that the supposed rabid dog-bitten in France had increased in extraordinary proportions: while, at the same time, the average mortality from rabies in France had shown but little fluctuation.

The arguments I have already given are such as will appeal at once to the lay mind. There are, however, certain scientific objections which are still more cogent. The action of the supposed prophylactic, when examined, resolved itself into pure empiricism. A number of injections of rabbits' spinal cords, that had been dried from fourteen to five days, were used, and the old *post hoc* argument was employed: because the children treated by these injections did not subsequently develop hydrophobia, therefore the prophylactic was the remedy. This simple proposition loses its force, however, when we note carefully what really happened. In one series, cords were used based on one formula. Some "cures" resulted, but deaths also occurred. Then the formula was altered, and made more intensive, with the result that a larger number of deaths occurred. A return was then made to the first formula, with some slight modification. Deaths still occurred. In explanation of the deaths, a general affirmation was made that the cases that died came "too late." But, in looking through the list of patients, I found that the cases which were "cured" were, in many cases, of just as long duration, and that with regard to them no assertion was made that they came "too late." Take, for instance, the case of Lord Doneraile. If, in his case, eleven days was too late for treatment, then all cases that

¹ Extract from an article by Thomas M. Dolan, M.D., in The Contemporary Review for July.

came after that period had elapsed must be expunged from the list of cures. Or, if Lord Doneraile's death was due to the application of the weak or first method, then the cases of the others treated by the same formula fall to the ground.

A yet further objection from the scientific standpoint is to be found in the fact that we know absolutely nothing of the action of these injections. M. Pasteur has not been able to tell us either the rate of absorption of each injection, or any thing of the physiological processes which take place.

In order to substantiate these objections, I shall throughout quote M. Pasteur's own words, and give his own statistics, and the names of his cases. The first point to which I must call attention is the remarkable classification which he has adopted with regard to the proofs that the patients he has treated were bitten by dogs that were really suffering from rabies. The following is the form adopted: Class A. Cases in which the dog was proved to be rabid by the experimental test; Class B. Cases in which the dog was recognized as rabid by the veterinary surgeon; Class C. Cases in which the dog was only suspected of being rabid.

This classification presupposes that all the patients have been exposed to danger. It makes no allowance for non rabid dogs, with the strange result, that, according to these statistics, a veritable epidemic of rabies affecting thousands of dogs must have existed in France during the years which have elapsed since M. Pasteur introduced his system.

It may be objected that in this matter the most eminent medical men in England support M. Pasteur. In reply to this, I will take one of the most eminent of these names, and show that his assertions with regard to the Pasteurian system are not supported by statistical, physiological, or pathological evidence. At a meeting at the Mansion House on Monday, July 1, 1889, Sir James Paget stated, that, "taking the average of all persons bitten by rabid dogs, 15 per cent would suffer from the disease, and 15 per cent would die. . . . In the seven thousand bitten, if fifteen of each hundred had died, there would have been as nearly as possible a thousand deaths, but only a hundred died. Pasteur has therefore saved already nine hundred lives." In the report of the English Hydrophobia Commission, Sir James Paget agrees with his co-workers in stating 'that at least 5 per cent of the persons bitten would suffer from the disease.' We have therefore two estimates of 5 and 15 per cent as the mortality in the case of those bitten by rabid dogs. With each percentage we obtain a supposed increased saving of life.

In order to realize the value of these assertions as applied to the Pasteurian statistics, let us inquire carefully what was the mortality in France during the years before M. Pasteur took up his work; for it will be manifest that if this system saved nine hundred lives in five years, then there ought to have been an equal or proportionate mortality in France before the introduction of the Pasteurian system. The facts, however, are far otherwise. The illustrious Tardieu, in a report presented to the minister of hygiene in 1863, maintained that twenty-five cases of rabies per year approximately represented the mean mortality in France from that disease.

The following figures represent the mortality in France in each year from 1850 to 1872: 1850, 27; 1851, 12; 1852, 46; 1853, 37; 1854, 21; 1855, 21; 1856, 30; 1857, 13; 1858, 17; 1859, 19; 1860, 14; 1861, 21; 1862, 26; 1863, 49; 1864, 66; 1865, 48; 1866, 64; 1867, 57; 1868, 56; 1869, 36; 1870, 36; 1871, 14; 1872, 15. But these statistics may be objected to on the ground of their antiquity. I therefore give more recent statistics, furnished by one who is not unfavorable to the Pasteur system.

Dr. Dujardin Beaumetz, director of the Sanitary Service of Paris, has addressed to the prefect of police a report on hydrophobia in Paris, furnishing the following data of comparison:—

Four Years antecedent to Pasteur.			Four Years of Treatment.		
		Deaths.			Deaths.
1882	.	9	1886	.	3
1883	.	4	1887	.	9
1884	.	3	1888	.	19
1885	.	22	1889	.	6
		38			37

These figures represent the deaths in the Department of the Seine.

With such figures before us, what becomes of the statement of our distinguished surgeon, that M. Pasteur has by his system saved nine hundred lives, when the national statistics of France prove conclusively that in no five years, for a quarter of a century, preceding M. Pasteur's discovery, did any such number of people die from this disease. The statistics of other countries also negative the statement.

The remarkable effect produced upon the statistics of hydrophobia by M. Pasteur's discovery is yet more strikingly shown when we investigate the returns from the Paris hospitals. On Nov. 2, 1887, M. Pasteur wrote to the Academy of Medicine as follows: "We know that sixty persons have died in the Paris hospitals during the last five years, a mean of twelve per year." After careful investigation of the hospital returns for those years, I am compelled to contradict this statement, and I am prepared to submit to M. Pasteur a full list of the names of all the patients who died in the Paris hospitals during that time, showing the following results: in 1881, 11 died; in 1882, 3; in 1883, 4; in 1884, 3; in 1885, 5. This gives a total of 26, or an average of 5.2 per annum, in place of the annual average of 12, as estimated by M. Pasteur. I am able to give the full details of the number of French patients treated by M. Pasteur who have died since the introduction of the Pasteurian system. For the complete list of the names of the patients, with the date of the bite, date of treatment, and date of death, I am indebted to the energy and ability of Dr. Lutaud. Space will not permit me to give this list in full, but the following is a recapitulation: in 1886, 19 deaths; in 1887, 27 deaths; in 1888, 23 deaths; in 1889, 21 deaths; giving a total of 80, or a yearly average of 20. These cases only represent the deaths after inoculation by M. Pasteur. To obtain the annual mortality of rabies in France, we must add to the foregoing the deaths of those persons who have not been treated at the institute. According to statistics published by M. Pasteur himself in 1886, the deaths among the non-inoculated for that year amounted to 17. If these be added to the 19 who died after treatment, we have an annual mortality of 36, as against an annual mortality, according to Tardieu's returns, prior to the introduction of inoculation, of 25 to 30. With these statistics before us, we are forced to the conclusion that the words quoted from the address of our eminent surgeon at the Mansion House were prompted more by generous impulse, and by feelings of respect and friendship for Pasteur, than by any strict regard to statistical data. As we have seen, Sir James Paget fixes the general mortality of those bitten at 15 per cent. M. Pasteur, in his article in the *New Review* (December, 1889) accepts this estimate, but thinks it is too low for bites on the face and other exposed parts. In such cases he thinks that the figures should be from 60 to 90 per cent. If we add up the number who have been bitten on exposed parts, and accept these percentages, then M. Pasteur's saving of life has been much greater, and his cures for France alone amount to some hundreds per annum. When we remember the ascertained mortality in France, and the rarity of hydrophobia there in past years, such percentages as the foregoing reduce the system to an absurdity.

The good old Dr. Berkenhout, writing about rabies in 1783, told us that he knew not of any human attempt which had a better resemblance to the Knight of La Mancha's attack on a windmill than that of combating popular errors and reasoning against popularly received opinions. I have been at times disposed to accept this view, and have felt inclined to let popular fashion expend itself. When I first criticised the method of Pasteur, what I said was received with incredulity and positive disfavor; but as time went on, and many of my predictions were verified, the incredulity gave place to greater tolerance in regard to opinions expressed against the system. There was a complete change of front. The infallibility of the method was abandoned, and its apologists adopted another tone. "Pasteur's system was not perfect," they said. "No system of therapeutics was perfect. Pasteur would be an angel, and not a man, if he could, at one *coup*, bring rabies into subjection." "Give him time," said another. Yet another apologist appealed to the law of averages, and said, "Pasteur has reduced the mortality from 5 per cent to 1 per cent."

To M. Peter the world owes the first exposure of the dangers of

the intensive method. It required great courage on the part of Dr. Peter, Dr. Lutaud, and others in France, and Dr. B. W. Richardson in England, to oppose the fashion. Had they not been actuated by a pure love of science, they would have been silenced. It is, however, not unsafe to prophesy that the intolerance of the new school in France, as shown in its treatment of Professor Peter, will bring about its own downfall.

Have we any treatment, then, that is satisfactory, based on these discoveries? The only answer to this question must be an emphatic "No!" The clinical observer has been very patient, knowing that he could afford to wait. Professor Peter, one of the greatest of contemporary clinical observers, and the worthy successor of Rousseau, has endeavored to save medicine from the reign of terror formed by the coterie which, in the name of science, anathematized all who ventured to doubt their theories. "You are unscientific," said the coterie; "you do not believe in our methods of modern research, and you cannot have a hearing." This kind of language has silenced many, because, when there is a fashion, men foolishly imagine that they will be looked on as progressive if they go with the tide. Martyrdom is not so eagerly sought after; and social ostracism is the penalty, too often, for appearing in a minority, as did M. Peter at the Academy. Clinical observers may, however, take heart: there are signs that the cloud will lift, and that medicine will yet be emancipated from the trammels of what has been so well called "vaccinomania."

BOOK-REVIEWS.

Advanced Physiography. By JOHN THORNTON. London and New York, Longmans, Green, & Co. 12°. \$1.40.

THIS book treats of advanced physiography as defined by the syllabus of the Science and Art Department of South Kensington, London. It embraces a concise statement of astronomy; an account of the size, shape, and density of the earth; a brief consideration of atmospheric and oceanic movements and of terrestrial magnetism; and some mention of certain other things on which questions might be asked in the science and arts examinations. Several sample examination-papers are appended, so that the student may, as it were, see what he is studying for. The examinations certainly are of value, and tend to turn school studies in directions approved of by competent educators; but, when it comes to writing a book to meet the examinations, the lover of pedagogics may well rebel. Mr. Thornton has done his task conscientiously. He has searched through good works for his materials, and has said something of every thing that the most ingenious examiner could ask about, and said it concisely and well, as a rule. He has avoided the staleness of old text-books, and has introduced many results of recent investigations; but, for all this, his book still leaves the impression of leading its students to South Kensington, rather than to good mental training. Moreover, the frequent wholesale quotation from other text-books gives the impression that the author is too greatly a compiler, and too little an investigator. In these modern days, when the preparation of school-books is considered worthy work for the director of the Geological Survey of Great Britain, for the superintendent of our Nautical Almanac Office, and for other eminent scholars, it makes us a little impatient to meet a book that is so distinctly a compilation as this one is; but perhaps we lay too great emphasis on this point. Books on physiography, as here defined, must be in great part compilations.

If not an investigator, the author is evidently a practised teacher; and his chapters, paragraph headings, and illustrations show an aptitude in methods of statement and explanation that must bear good result. The careful account of the different methods of finding the masses of the planets, the full description of modern spectroscopy, and the extended chapter on comets and meteors, may be cited in evidence of this. The accounts of the tides and of the winds are distinctly less successful. Occasional lapses appear, such as, "Heat and light are forms of radiant energy," or as latitude being shown as an angle at the centre of the earth, or as making our tornadoes identical with West Indian hurricanes and Chinese typhoons; but errors of even this minute

kind are not common. Condensation of statement in certain chapters will either leave much work for the teacher, which is not objectionable if he is equal to it, or will leave the scholar in a very confused state of mind; and this leads back to our starting-point, that a book prepared to enable students to meet examinations is not the best kind of a book for securing intellectual training.

Graphical Statics. By LUIGI CREMONA. Tr. by Thomas H. Beare. Oxford, Clarendon Pr. 8°. (New York, Macmillan, \$2.25.)

THOSE who are accustomed to make use of mathematics as a tool, and who are not able to ascend into the higher regions of pure mathematics farther than is necessary to secure their practical ends, especially the engineer seeking the solution of the problems in kinematics and in mechanics that come to him in the course of his regular professional work, often have occasion to remark upon the extremely limited range of problems which are capable of solution by algebraic processes, and upon the greater effectiveness of the geometric methods. A glance at any treatise, on any branch of engineering, will show how narrow is the field of application of the algebraic systems to the practical work of the constructor. Where the elements are few, the conditions very simple, and the results sought similarly easy of expression, algebraic methods come in play; but, as in astronomy, the introduction of a little wider generalization, of a single new condition, often carries the problem entirely outside the field of application for the algebraist. Algebra does marvellous work, but its limits are soon reached. Graphical methods are often found to be far more satisfactory, not only in their ease of application, but in the readiness with which the results may be comprehended and translated into the language, and represented by the work of every-day practice. Thus it happens that "graphical statics" has come forward, within a very short time, as the most valuable tool of the engineer.

The father of the system, in some sense, is the well-known Culmann, whose treatises have been translated into English by Dubois; but some work had been done even before he attempted collating and systematizing it. Rankine did much in this field; and many minor writers have added, each his mite, to the subject. We observe that Cousinery is credited with many contributions to the subject by the writer of this latest treatise. Professor Cremona begins by the presentation of the system of signs adopted, in which he follows Moebius, and then takes up the work in the usual way, giving the standard methods of arithmetical treatment, the graphics of the four rules; the discussion of the processes of graphical involution and evolution; the solution of numerical equations; and the discussion of the centroids. The second part consists of a discussion of reciprocal figures, including Rankine's theory of structures and polygonal frames, and Culmann's work in the same department. The work is well written, the system satisfactory, and the methods in detail logical and exact. Professor Beare is entitled to commendation for his admirable translation; and both he and its author deserve much from the English-speaking reader and student of "Graphical Statics."

Like all the work of the Clarendon Press, the book-making is excellent, and deserving of all praise.

Cycling Art, Energy and Locomotion. By ROBERT P. SCOTT. Philadelphia, Lippincott. 12°.

THIS is an interesting little 12mo treatise on the art of the wheelman, which, in a space of three hundred pages, gives a good historical summary, and an account of the later forms of the wheel, and of the principles of their construction and operation, and presents the mathematical and scientific principles of their balancing and propulsion. One of the most interesting chapters in the book is that in which the author gives the graphical measurements obtained by him with an autographic apparatus devised by himself to record the resistances to the motion of the machine and the pressures of the foot on the pedal. Exact knowledge on these points has not heretofore been obtainable, and this investigation is a real contribution to our knowledge in this field. The